DOCUMENT RESUME

ED 193 284 TM 800 599

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TITLE Intelligence Testing 1928-1978: What Next?
INSTITUTION Scottish Council for Research in Education.
PUB DATE 79
NOTE 35p.: Paper presented to the Fellowship of the Scottish Council for Research in Education (Edinburgh, September 8, 1978).
AVAILABLE FROM The Scottish Council for Research in Education, 15 St. John Street, Edinburgh EH8 8JR, Scotland (10.70)

EDRS PRICE MF01 Plus Postage. PC Not Available from EDRS.
DESCRIPTORS Cognitive Development: Educational History: *Intelligence: Intelligence Quotient: *Intelligence Tests: *Nature Nurture Controversy: *Testing Problems: Test Reliability

ABSTRACT

Attention is drawn to the ways in which current conceptions of intelligence and its measurement differ from those which were generally accepted in 1928. The following principles underlying intelligence testing were generally agreed upon in 1928: (1) the assumption of intelligence as a recognizable attribute, responsible for differences among children and adults in learning, reasoning, and other cognitive capacities: (2) the principle that sampling appropriate mental tasks and norming scores against distribution in the population yields IQs acceptable as quantitative measures of level of intelligence: and (3) essentially innate and genetically determined, intelligence develops with age, regardless of the environment. These statements are contrasted to 1978 thinking, and the following common criticisms of intelligence testing are highlighted: most item types are developed haphazardly without clear rationale: group tests are often given by untrained lay people under poorly controlled conditions: test results depend on practice and motivation: the tests are unreliable, and, the public feels threatened by tests. The remainder of the paper is a look at recent investigations supporting genetic/environmental factors on intellectual growth. (GK)

TM 800599

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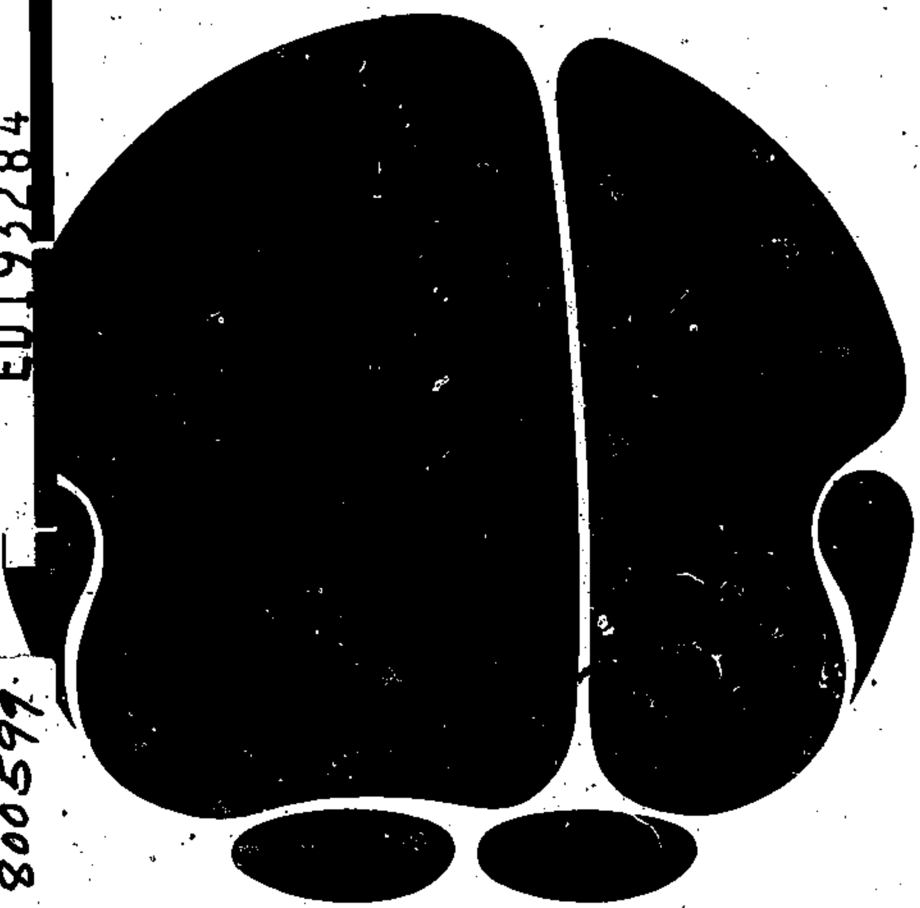
INTELLIGENCE TESTING 1928-1978 WHAT NEXT?



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Philip E Vernon

INTELLIGENCE TESTING 1928-1978: WHAT NEXT?

A paper presented in Edinburgh on
8th September 1978
on the occasion of his being awarded the
Fellowship of the Scottish Council for
Research in Education

by

Philip E Vernon

The Scottish Council for Research in Education

ISBN 0 901116 16 5

Printed and bound in Great Britain by Lindsay & Co Ltd,
16 Orwell Terrace, Edinburgh, EH11 2EU.

PHILIP E VERNON

That the Fellowship awarded by the Scottish Council for Research in Education — an honour restricted to Scots or those who have worked in Scotland — should have been made to one who last worked in Scotland in the mid-1940s may at first sight be surprising. To those who have come under his influence, however, it can be no surprise that Philip Vernon was the immediate and unhesitating choice of the Scottish Council for Research in Education as the first one to receive its senior honour. The paper contained in this booklet stands as evidence of the rightness of this choice.

Philip Vernon's time in Scotland was spent in Glasgow. He first went there in 1935 after a most outstanding studentship at St John's College, Cambridge, and at Harvard, and after a period as a psychologist at the Maudsley Hospital, London. (His teachers had included Spearman, Burt, MacDougall, Stout, F C Bartlett, and Allport.) And it was while he was Principal Lecturer in Psychology at Jordanhill Training College that he produced his first book, *The Measurement of Abilities* (1937). Though the war took him away from Glasgow to work on personnel selection in the Armed Forces, he returned there for a short time after the war, making his mark in the Department of Psychology at Glasgow University. On leaving Glasgow he again worked with the Services as a senior psychologist in the Admiralty, but he soon moved back to academic life when — in 1949 — he was appointed to the Chair of Educational Psychology in the London University Institute of Education. In 1968 he left London for Canada, where, for ten years, he was Professor of Educational Psychology in the University of Calgary. He has recently retired from his university duties there.

The present paper, though following a line of publications devoted to the critical summarisation of the work of others, repre-

sents only one side of his contribution to educational psychology. He has shown originality of mind in work ranging from intelligence tests (produced while he was still a student at Cambridge) and studies in psychology of music (while a PhD student) to his mature work on the causes — social, political, and educational — of backwardness in many societies.* Not least amongst his contributions to education has been his influence on students: the intellectual rigour of the present paper gives some indication of why that influence has been so great.

* *Intelligence and Cultural Environment* (1969).

INTELLIGENCE TESTING 1928-1978: WHAT NEXT?

I particularly appreciate the honour of being invited to give the first lecture celebrating the Fiftieth Anniversary of the founding of the Scottish Council for Research in Education, since I lived and worked in Scotland from 1935-1947 (apart from four of the war years). And though I was not directly connected with the Council's work, I knew Dr Rusk and Sir Godfrey Thomson very well, and greatly admired the steady stream of researches which the Council encouraged and published. While I was naturally most interested in the Scottish Mental Survey volumes, it seemed to me valuable that historical and other types of educational research should be fostered. And in recent years, during which more of the research has been conducted by professional staff, less by individual educationists, I have welcomed the greater emphasis on problems connected with school organisation and curriculum rather than on psychometric studies.

My choice of title is not meant to imply that I am going to give a detailed historical account of developments in mental testing over 50 years. Rather I wish to draw attention to the ways in which current conceptions of intelligence and its measurement differ from those which were generally accepted in 1928. Also the title refers to the fact that I myself first became interested in this field in 1928, as a PhD student at Cambridge University, and produced my own first intelligence test for high-grade secondary and college students. Thus these 50 years also cover my own active research and writing on the topic.

In 1928, Spearman, Burt, Drever and Thomson reigned supreme in British psychology. The Stanford-Binet scale for children had come into general use; group tests had demonstrated their value in the American army; and many such tests as the Moray House series were available for children. Despite the bitter controversies between Spearman and Thomson, there was fairly

general agreement on the following principles underlying intelligence testing.*

First it was assumed that intelligence is a recognisable attribute which is responsible for differences among children and adults in their learning, reasoning, and other cognitive capacities. It is an homogeneous entity or mental power which, like height or weight, can vary in amount, or in rate of growth or decline, but is essentially stable in its nature throughout life. Secondly, although obviously it is not measurable in the same sense as physical attributes like height, yet the principle of sampling appropriate mental tasks and standardising or norming scores against the distribution in the general population yields IQs which can be accepted as quantitative measures of level of intelligence. Thirdly, intelligence is essentially innate, being determined by the genes that the child inherits from his parents; hence it develops or matures with age, irrespective of the environment in which he is reared. It reaches its maximum by around 15 years and then stays constant until senility sets in. Thus the IQ obtained from a reliable intelligence test in childhood indicates the educational and vocational level that the person can be expected to attain in his later school career and in adult life. Burt, writing on general intelligence in 1933, said: "Fortunately it can be measured with accuracy and ease".

But in 1978 all these statements, though perhaps containing some grain of truth, would be hotly contested by the great majority of psychologists. How is it that the testing movement, long regarded as a major achievement of applied psychology, and accepted by most laymen as veridical, is now so widely distrusted and criticised, and is even in some danger of abolition in the United States, where it once flourished most luxuriantly? Several States have passed, or at least considered, laws to ban the use of IQ tests in schools, on the grounds that they are culturally biased and do not accurately measure intelligence. Many American parents have successfully challenged in the courts the allocation of their children to special schools or classes on the basis of low IQs. It has also been ruled in some suits that employers cannot refuse

* This, and some subsequent, paragraphs are largely quoted from a forthcoming book: *Intelligence: Heredity and Environment*, to be published by W H Freeman, San Francisco.

to employ blacks or others who obtain low test scores, unless there is clear evidence that suitability for the job depends on what the tests measure. The main attack has been on large-scale group testing, and there is less interference as yet with the use of individual tests for clinical diagnostic purposes. But the latter too has been criticised, and some school psychologists have been forced to substitute the Illinois Test of Psycholinguistic Abilities, or tests of concept formation or of Piagetian stages, which measure much the same thing rather less effectively and conveniently but which avoid the naughty word "intelligence". I need hardly mention the similar decline in group intelligence testing in the UK with the virtual demise of the 11+.

In the early 1920s the average scores of American army recruits of different national or ethnic descent showed that men of Anglo-American or Northwest European descent exceeded those of Southern and Eastern European stock; and American negroes scored lower still. But the objection was soon raised that these differences resulted from the economic and educational advancement of the different groups rather than from innate differences in ability. In 1928, Freeman, Holzinger and Mitchell's, also Burks's, studies of foster children were published, suggesting that the adoption of orphans into good foster homes brought about significant, even if limited, improvements in their mean IQs. The work of Hugh Gordon (1923) with canal boat and gipsy children, also of N D M Hirsch (1928) with children living in isolated rural regions of Kentucky, strengthened the suspicion that IQs were more susceptible to environmental advantage or disadvantage than such pioneers as Terman and Burt believed. Then in 1937, Newman, Freeman and Holzinger published the first investigation of identical twins reared apart, which demonstrated that the correlation among pairs was higher than that among non-identicals (thus demonstrating genetic influence), yet it was considerably lower than the correlation among pairs reared together (presumably on account of their different environments). Figure 1 shows Erlenmeyer-Kimling and Jarvik's collection of correlations between related pairs, and the bottom four rows clearly illustrate the effects of genetic and environmental factors. At about the same time, R L Thorndike (1933) pointed out that IQs are much less stable over time than was generally believed. Although

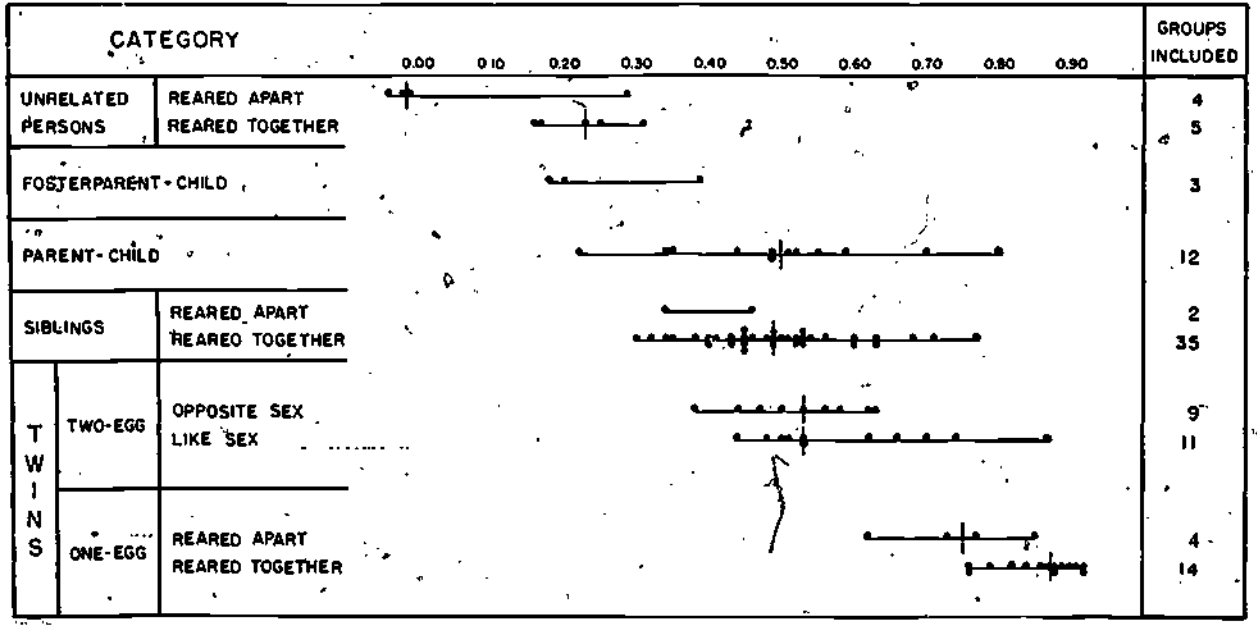

Figure 1. Correlations between IQs of Various Kinship Pairs, from 52 Studies (Erlenmeyer-Kimling, L., and Jarvik, L. F., "Genetics and Intelligence: A Review", *Science*, 1963, Vol. 142, p. 1478).*

individual tests given a week apart correlated at about .95, the reliability coefficients over 5 years dropped to around .70. Thus intelligence was by no means fixed for life; and the later longitudinal studies by Bayley (1949) and others showed that developmental or intelligence quotients obtained in the first 3 years or so of life, bore scarcely any relation to later childhood or adult IQ (see Table 1).

Table 1

Correlations between IQs from Various Preschool or Intelligence Tests, at Different Ages (from N Bayley).

Age at first test	No. of Years until Retest			
	1	3	6	12 yrs.
3 mths.	0.10	0.05	-0.13	0.02
1 yr.	0.47	0.23	0.13	0.00
2 yrs.	0.74	0.55	0.50	0.42
3 yrs.	0.64	—	0.55	0.33
4 yrs.	—	0.71	0.73	0.70
6 yrs.	0.86	0.84	0.81	0.77
7 yrs.	0.88	0.87	0.73	0.80
9 yrs.	0.88	0.82	0.87	—
11 yrs.	0.93	0.93	0.92	—

In 1949, Hebb's influential book, *The Organisation of Behaviour*, seemed to provide a reconcilation between the opposed views of hereditarians and environmentalists, by distinguishing what he called Intelligence A and Intelligence B. Intelligence A was the genetically determined plasticity of the central nervous system which was necessary for any mental growth; but this could not be observed directly nor measured. What we do observe and measure fairly effectively is the current level of all-round mental efficiency. Intelligence B, and this depends on the interaction between the genetic potential and the stimulating or inhibiting effects of the environment in which the individual has been reared. Hebb's experiments on dogs and rats reared in different environments supported this interpretation. At about this time too, Piaget's work on child development was becoming widely known in Britain and America, and his book on the *Psychology of Intelligence* (1950) likewise stressed the role of physical and social environment, as well as neurological maturation, in the development of operational thinking. This interactionist theory is now

accepted by almost all psychologists except for a few rabid environmentalists; but unfortunately, of course, it still leaves scope for a wide range of opinions on the relative importance of hereditary and environmental determination. I will describe later some of the more recent and convincing studies of this problem.

The other main line of attack on intelligence testing came from the factor analysts who, following Thurstone and Guilford, claimed that intelligence is not a unitary general ability, but a collection of numerous independent kinds of ability or primary factors, Verbal, Spatial, Number, etc. Now the multifactorial model works quite well when batteries of varied tests are given to selected, homogeneous populations such as college students. But as Thurstone found when he extended his investigations to younger age groups, one or more second-order factors appeared which, he admitted, corresponded to Spearman's 'g'. In an heterogeneous population such as Army recruits, or an age group of children, the general factor accounts for at least 50 per cent of the variance, and additional more specialised abilities for not more than about 25 per cent. Burt has shown that the 'g'+ group factor model is mathematically equivalent to the Thurstone multiple factor solution. They are just alternative, and mutually convertible, ways of classifying abilities. Thurstone's view appeared to be superior in practical utility because it provided a profile of scores on half-a-dozen or more separate ability factors, rather than just the single global IQ plus some small additional group factors. But the trouble is that the differential or pure-factor scores derived from Thurstone's model just did not differentiate. Far too many testees score high on all of them, or low on all. Thus we find in practice that, despite all the efforts put into factorial investigations and techniques over the past 70 years, almost all applied psychologists working in school, clinic, or occupational fields make use chiefly of a single general test, or, quite often, of a two-pronged test like Wechsler's verbal and performance, or the Verbal and Nonverbal Lorge-Thorndike or Alice Heim tests, or the verbal and quantitative College Entrance Board tests. Numerous critics of intelligence testing allege that these general tests are worthless because different factorists put forward contradictory theories of ability structure; and there is no agreement on the nature of intelligence or 'g' (cf. Block and Dworkin,

1974). But in fact the general factor is so prominent that quantitative studies of heritability do give pretty consistent results: whereas attempts to study the genetic and environmental components of Thurstone's primary factor measures have yielded hopelessly contradictory results.

Turning now to the common criticisms raised by people who oppose testing, whether for ideological or other reasons: and how far can these be answered? It is often easy to pick out particular test items for ridicule, and to say that these don't measure "real" intelligence. Actually, of course, all items will usually have been validated against total score, though it is true that some get out-of-date and need revision, eg "In what way are coal and wood alike?" Though we can't reach any precise operational definition of intelligence, the evidence from factor analysis and other sources does show that items are measuring a consistent and important component of ability. But I would have to agree that most of the item-types have developed haphazardly without sufficiently clear rationale. Perhaps we could arrive at a better sampling of mental efficiency through advances in our knowledge of information processing, as Resnick and her co-authors suggest in *The Nature of Intelligence* (1976).

Second: group tests are often given by untrained laymen, under poorly controlled conditions; they may be mis-scored and misinterpreted. With this I would tend to agree. Although certainly I have made much use of group tests, their results have always seemed to me too chancy to be of great value in individual diagnosis. And this is borne out by the considerably greater stability of individual test IQs over time. For example, the correlation of Binet 11-yr. IQs with 17-18 yr. IQs is about .85 to .90; whereas verbal group tests correlate .75, and nonverbal group tests about .60 (see Table 2). Individual testers are, of course, better trained and can standardise administration more adequately, though they too have been shown to have their idiosyncrasies, such as varying standards of evaluating children's responses. And it is highly probable that they are susceptible to halo or expectancy effects: that is they judge from what the school tells them, or from impressions obtained in initial conversations, that the child is bright or dull, and this influences their administration and scoring. On the other hand there is no justification whatever for

the teacher expectancy effects alleged by Rosenthal and Jacobson (1968). Their experiment purporting to show that children who are reported to teachers as being bright obtain significant IQ gains over the next few months, was full of technical faults. And numerous attempts at replication have led to completely negative results (see Elashoff and Snow, 1971).

Table 2

Correlations between Verbal and Non-verbal IQs at different Grade levels and Terminal IQs (Hopkins and Bracht).

Grade	Verbal	Non-Verbal	Combined	Binet
1	52	32	53	(.66)
2	.59	.51	.66	(.72)
4	.75	.56	.72	(.78)
7	.74	.65	.77	(.82)
9	.78	.67	.82	(.80)

Third: test results depend on practice or coaching. Agreed that significant rises have been demonstrated; but they are limited, and they can be largely overcome by giving all children concerned adequate preliminary practice. The individual tester can usually guess when the same test has been used recently. Where this weakness becomes really serious is in cross-cultural group testing of children or adults who have no previous experience of tests.

Fourth: test results depend on motivation. Obviously, it is said, the child who is confident, cooperative and interested will do better than one who is anxious, bored, distractible, or has a negative self-concept. Actually it has been difficult to get much confirmatory evidence; except in child guidance cases, where the tester can gauge the child's cooperation, and can do a good deal to build up suitable rapport. She will also usually note if the IQ is likely to be unreliable through lack of motivation. But the group tester, I must admit, can do very little to stimulate the motivation of all the children in a class. Some experiments (eg Benton, 1936) have indicated that extra motivation has little effect; for instance offering monetary rewards for good performance may stimulate children to try more items, but not to get more of them right. We also found in the army that the scores of women recruits were not affected by minor illnesses such as colds, feeling off colour, or menstrual periods. I would agree that this point too is of much

greater importance in cross-cultural testing, where the testees may be suspicious or anxious about a foreign tester.

Fifth: I suppose the most common and most misguided criticism of intelligence tests is that they merely measure acquired information and skills. Items such as those shown from Binet or WISC in Table 3 are often quoted, with the implication that

Table 3

Some Specimen Individual Test Items

1. Who wrote Romeo and Juliet?
2. What is a hieroglyphic?
3. What is the thing to do if another boy (girl) hits you without meaning to?

obviously slum children have had much less opportunity than middle-class children to hear about the first two items; while the third represents knowledge of moral conventions in middle-class society, and the typical response from lower-class children might be very different. But it is false to say that vocabulary and verbal skills are acquired, in the sense that anybody could acquire them, if taught. They are developed in the same way as other aspects of Intelligence B. Children don't usually know or use difficult words unless they have reached the level of mental maturity sufficient to understand the concepts to which the words refer. Actually E L Thorndike published a careful investigation of just this point in 1927. He gave 3 tests involving vocabulary and informational skills, and 3 tests involving verbal and mathematical reasoning skills, to Grade 8 boys, and found that the correlations between the two types of test were just as high as those within either type. In other words the information tests were measuring reasoning capacity as effectively as did tests designed to sample reasoning. However I would agree that most test constructors nowadays do tend to avoid items which seem to involve cultural bias, and to rely more on items whose difficulty depends on complexity of information processing.

Because of the importance of this topic, I will quote a more recent study by Arthur Jensen (1974) of test bias. The Peabody Picture Vocabulary, which appears likely to be culturally loaded, and the Raven Matrices, which appears relatively culture-fair, were given to 600 white and 500 black pupils in Californian

schools. On both tests the whites, as usual, obtained higher mean scores, the black performance being equivalent to that of whites a year or two younger. But there were no other features of the responses to indicate any differences between the groups. The reliabilities and the factorial content of tests and items were the same in blacks and whites, and the rank orders of item difficulty were almost identical. That is, there were no items that were relatively more or less difficult for one group than the other. Also the blacks were not handicapped more on vocabulary than on Matrices. Many other studies have been carried out by various authors with college students, showing that Scholastic Aptitude and other types of intelligence tests predict college grades among blacks in just the same way as they do among whites (see Hunter and Schmidt, 1976). The tests are more difficult for low economic class or minority group students, and this difference is doubtless in part due to deprived conditions of upbringing. But it is just not true that the tests are unfairly loaded against particular subgroups reared within western societies.

My sixth point arises from the admittedly imperfect reliability of intelligence tests. Critics often report atrocity stories about children whose educational and vocational careers were permanently blighted because they obtained a low IQ soon after entry to primary schooling. I would not say that this cannot occur (particularly if group tests are used), but no one has compared the incidence of faulty prognoses as against the incidence of valid ones, where the information obtained by an intelligence test has led to much more appropriate educational provisions than merely relying on parents' or teachers' judgments. In our Calgary school system, it is usual for special class or special school children to be retested individually every 2 years, in case they do show sufficient improvement to justify return to ordinary schooling.

Last of these common criticisms: Cronbach (1975) points out that the very success of intelligence and educational testing has contributed to their downfall. The public feels threatened because more and more of their own, or their children's, educational and occupational careers are decided by tests, whose content, and the scores they yield, are kept secret. Many people would prefer the old-fashioned approach of school examinations, interviews, and decisions based on academic and occupational record, with which

they are familiar, and it is difficult to convince them that these are even less accurate. Then too there is the revolt against invasions of privacy at a time when so much personal information is being computerised; thus many parents demand access to the psychologist's files. Whereas the psychologist regards this information as confidential as the doctor does; and he refuses to give out IQs or other scores because they would so readily be misunderstood. Further, how could he carry out clinical examinations or treatment without keeping records of family circumstances that the parents might resent? I would like to add too that psychologists such as Anastasi (1968), Cronbach (1970), Sattler (1974) and others are very well aware of the defects and dangers of testing to which I have drawn attention; and they do their best by their textbooks and university courses to train teachers and future psychologists to improve their usage of tests.

I will now go on to some of the major investigations of recent years which supply strong evidence of the influence both of genetic and environmental factors on children's intellectual growth. On the genetic side, the greatest weight has been put on kinship data, especially on identical twins reared apart. In four separate studies*, which yielded only 122 pairs in all, the mean inter-twin correlation was .82, and this figure can be taken as an estimate of the heritability of IQ, or percentage of genetic variance, the remaining 18 per cent being environmental. However the largest single group was the 53 pairs collected by Sir Cyril Burt; and his correlation of .88 suggested that separated identicals are almost as much alike in intelligence as identicals brought up in the same home. In 1972, L J Kamin, of Princeton University, drew attention to certain discrepancies in Burt's published figures, and in 1974 Jensen issued a complete list of all such apparent inconsistencies. Several of these were probably miscopyings, but others were more serious, such as reporting identical correlations coefficients for different sized groups of twins, suggesting that Burt had not bothered to recalculate when he gathered additional cases. These lapses have been blown up by Kamin (1974), and by Gillie (1976) in the UK, into an accusation

* Namely Newman, Freeman and Holzinger (1937), Shields (1962), Juel-Nielsen (1960) and Burt (1966).

that Burt's data were faked, and that none of his findings are of any scientific value, since we cannot know whether they contain further distortions. I cannot myself regard Burt's work as fraudulent; almost all the discovered inconsistencies are so stupid that he would surely have made his results much more plausible if he had been intentionally faking. I would agree that he was more careless about such details than research psychologists are nowadays; also that he was so strongly wedded to genetic explanations that the planning of his investigations, and his methods and analyses may have been biased at times. For example, the techniques he used for assessing the intelligence of adult relatives, and the procedures of getting what he called children's adjusted IQs, seem to have been highly subjective. I'm afraid then that we will have to jettison his individual test data; but this does not mean, as the critics claim, that the whole foundations of heritability of intelligence are washed away. Other, more scrupulous investigators have obtained figures which do not differ significantly from Burt's; and the fact that his correlation for separated identicals is higher than anybody else's could very well be due to his pairs being children, whereas other workers like Newman and Shields used adults with a wide age range, whose test scores, therefore, would certainly be less reliable.

Kamin's book, *The Science and Politics of IQ* (1974), does not merely attack Burt. He tries to pick holes in all other published studies that suggest genetic effects; and several reputable reviewers have rejected his methods and conclusions. If Burt was biased in one direction, Kamin is much more so in the opposite direction. However he does make a very valid point about separated twin studies, namely that such twins are never randomly assigned to different parents. Although Burt himself denied it, I am sure that the two members of any pair would usually be reared in homes quite similar in socioeconomic and educational status. Hence part of the high correlation within such pairs could be due to environmental, and not only genetic, similarity. More generally, what is called Genetic-Environmental covariance has been neglected by most authors of heritability analyses. This term refers to the obvious likelihood that intelligent parents who pass on superior genes to their offspring also usually provide above-average environments. However 3 recent studies of

kinship data by Morton (1972), Loehlin et al (1975), and Jensen (1977) have separated off this component, and you can see from Table 4 that there is fairly close convergence on a genetic percentage of around 65, environmental 23, and covariance 12. Note too that the genetic percent is well below the 80 per cent which Jensen advocated in 1969 on the basis of Burt's group test figures. I think it quite likely that if we could test larger samples under more carefully controlled conditions, the genetic percentage might drop below 60. But the precise figure doesn't matter, so long as it is recognised that both the genes and the environment have very substantial effects on child IQ, and that the genetic component is probably the larger one, which we cannot afford to ignore. In any case Jensen and his critics point out that this figure, be it 80 per cent or 65 per cent or 50 per cent, is not an absolute one; it is relative to the particular population such as British or American whites, and would alter if the range of environmental differences altered. I have also neglected the complications of Dominance effect and of Assortative Mating, since these do not affect the main argument.

Table 4

Heritability Analyses Including Estimates for Genetic-Environmental Covariance

	Jencks	Loehlin et al.	Morton	Jensen
V_G	.45	.61	.68	.65
V_E	.35	.23	.19	.28
G × E Cov.	.20	.15	.14	.07

In view of the many difficulties associated with twin or other kinship data, the chief alternative approach is through foster children, since here we can study environmental effects without any genetic connection between child and foster parent. Unfortunately there are still a lot of complications, as Munsinger (1975) pointed out in a recent review; and the results of different investigations vary greatly. However there is a fair consensus that adoption at an early age into a good home tends to raise the IQs of adoptees, though probably not more than about 10 IQ points on average. The correlation between child IQ and measures of foster-parent ability or education, or the home rating, are mostly quite small. From 6 published studies (see Table 5) I find a

median figure of 0.23, and this is almost certainly boosted by the tendency for selective placement, that is the attempt by the adoption agency to match the child with the foster home level. In some studies it was possible to get estimates of the ability of the true, or biological, parents, though the data are seldom complete. In another 6 studies (see Table 6), I found a median figure of 0.30 (or probably higher). In spite of Kamin's obsessional efforts to demolish the published investigations, we must, I think, conclude that the genetic influence of true parents who did not rear the children is greater than the environmental effects exerted by the foster parents. So this line of evidence confirms rather neatly the conclusion from kinship analyses.

Table 5
Correlations of Foster Child IQ with Foster Parent Ability, or Home Level

	N	r
Freeman et al.	401	.39 to .52
Burks	214	.23 to .42
Leahy	194	.18 to .24
Skodak & Skeels	139	.04 to .20
Horn & Loehlin	146	.09 to .15
Munsinger	41	−.14
Approximate median		< .23

Table 6
Correlations of Foster Child IQs with Ability of Biological Parent(s)

	N	Age of testees	r
Munsinger	41	8½	.70
Skodak & Skeels	63	13½	.38 to .44
Horn & Loehlin	146	?	.32
Lawrence	185	9-14	.26 (with father SES)
Skodak & Skeels	139	7½	.23
Snygg	70	5+	.12
Approximate median			> .30

There are a number of other types of evidence which contribute to the case for genetic determination of intelligence: the fact that rats and dogs can be bred to produce bright and dull strains; the tendency for close human inbreeding to yield congenital malfor-

mations and mental defect; and the discovery that specific gene anomalies produce psychological syndromes such as Down's and Turner's. A point which strikes me as highly convincing is that children often do *not* resemble their parents or their siblings in intelligence. Resemblances between them (amounting to an average correlation of 0.5) could plausibly be attributed to the effects of common upbringing. But the fact that professional parents can have quite dull children, and lower-class, poorly educated parents have very bright children would be expected on genetic though not on environmental theory.

Let us now look at some of the more striking studies of environmental effects. Some remarkable investigations in recent years by Trevarthen (1974) at Edinburgh, also Schaffer (1977), Bower (1974), and Jerome Bruner (1975) have brought out the importance of mother-infant interactions in promoting cognitive development. Also longitudinal follow-up investigations like the Berkeley Growth Study, and the work at the Fels Institute, have yielded substantial correlations between certain types of parental handling of children, and their later intelligence. However such results are difficult to interpret, since they might also be explicable genetically. I have mentioned already the substantial genetic-environmental covariance effect. In addition, children with superior genes tend to exploit and even shape their own environments more effectively than dull ones. They are more interested in reading, ask more questions, and explore more actively. In other words, the genes may affect the environment rather than the environment affecting the intelligence. Hence the evidence from intervention studies is more convincing, where children are submitted to specially stimulating or deprived environments, and can be compared for intelligence with control groups.

Several cases of severe deprivation have been described in the literature, that is children who grew up in a highly restricted environment and had hardly any contracts with other human beings. As would be expected from interactionist theory, their mental development scarcely went beyond the imbecile level. Nevertheless Koluchova's (1972) pair of twin boys, and other cases, who were rescued and put into good foster homes as late as 7 years of age overcame their retardation. Their IQs rose from 40 or below to 100 or above. Thus a highly unfavourable environ-

ment in early childhood does not necessarily bring about irremediable damage.

In the late 1930s Skeels tested 24 orphans in a very unstimulating institution, at ages 7 to 30 months. Thirteen of them were then transferred to a hospital where they received a lot of care and individual attention from mentally defective girls; and most of them were later fostered out into lower middle-class homes. Skeels claimed a large improvement averaging 27½ IQ points in these transferred cases, whereas in the other 11, left in the original institution, there was a further drop of 26 points. Some 25 years later Skeels (1966) traced all these cases, and found the transferred ones to be normal, self-supporting adults, holding quite a range of skilled jobs, or else they were married women. The non-transferred were still either institutionalised, or in very low-grade jobs. The average number of years of schooling of the two groups were 11.7 and 4.0 years respectively. Though I would not put any credence in intelligence tests given at such an early age, it seems reasonable to conclude that, as adults, the transferred cases averaged at least 30 IQ points higher than the others.

Probably the best controlled study is that of Heber and Garber in Milwaukee (cf Garber and Heber, 1977), though insufficient details have so far been published for us to evaluate the findings. Forty negro boys were selected at birth whose mothers scored 80 IQ or below, and who lived in a very poor neighbourhood. Twenty of them — the Experimental group — attended a Centre for 7 hours a day, 5 days a week, and underwent an all-out programme devised to improve their sensori-motor, language and thinking skills. Simultaneously their mothers were given an educational programme including home-making, child care, and vocational training. The other 20 were brought up at home, but took the same periodic tests as the Experimentals. Up to about the age of 14 months the two groups remained closely parallel on the Gesell scale; but the Controls began to fall behind after 18 months (see Figure 2). On preschool scales given between 2 and 4½ years, Heber found mean IQs of 122.6 and 95.2, that is a superiority of 27.4 points among the Experimentals. Up to the age of 6 the Experimental means stayed between 110 and 120, whereas the Controls dropped to around 85. The special programme ceased when the children entered first grade. By ages 8 to 9, the Experi-

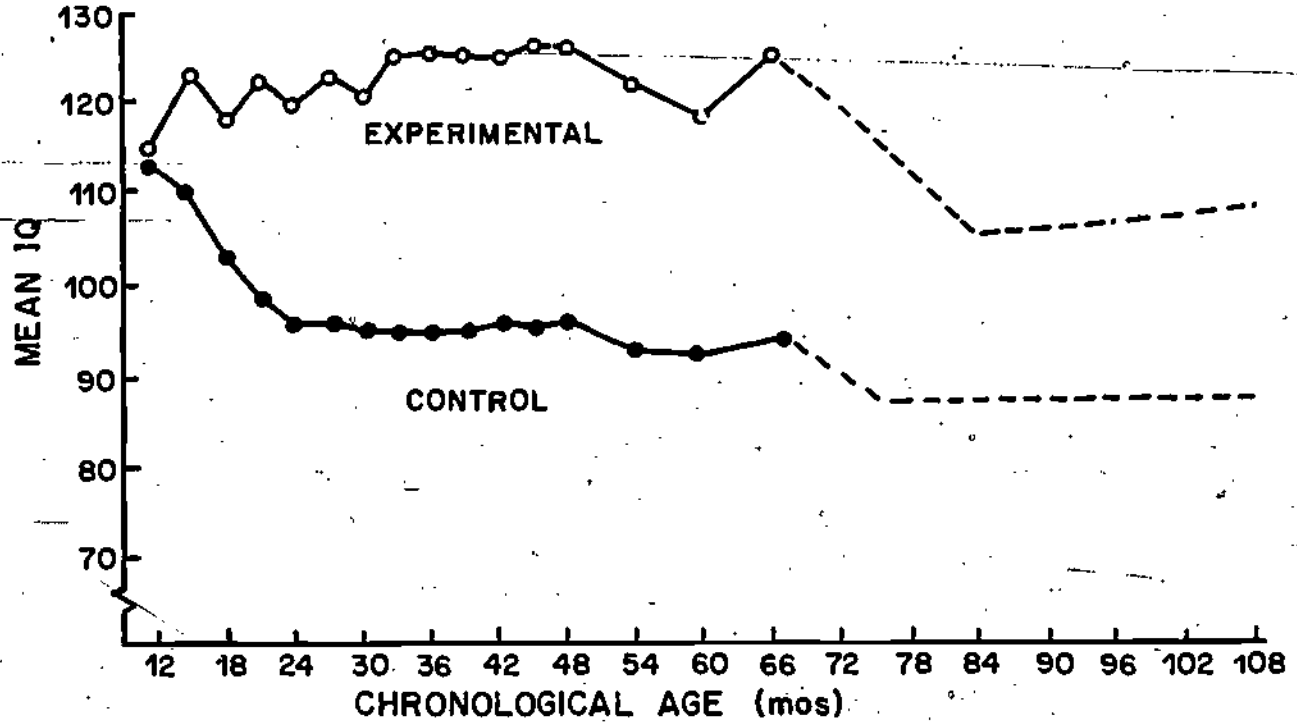

Fig. 2. IQs of Heber and Garber's Experimental and Control Groups.
(Note: The last two points at 7 and 9 years are approximate only, being based on a lecture, not a published article.)

mentals dropped to an average of 104, while the Controls now averaged 80. Final figures are not yet available and, in the absence of further stimulation, it is possible that the Experimentals may show some further decline. But clearly they have a tremendous advantage over the Controls reared in their own homes.

These results are all the more striking when compared with the virtual failure of the well-known Head Start experiment in the 1960s to produce any permanent gains. However, the Head Start programmes usually amounted only to a few months' attendance at a nursery school for a few hours a day, before entering elementary school. Since then, numerous intervention studies have been based more on helping the mothers to stimulate and interact better with their 1 to 4 year old children, by means of home visits from psychologists or specially trained teachers. Levenstein's (1970), Karnes and Teska's (1970), Bronfenbrenner's (1974), and other programmes have produced gains of some 13 to 20 IQ points, though there is not much evidence yet of the permanence of the effects. Another advantage of such schemes is that they are relatively inexpensive, probably costing less than Head Start; whereas the Milwaukee experiment was far too expensive ever to be applied to large numbers of children.

We might expect even more effective intervention if black, or American Indian, children, for example, were adopted shortly after birth by white parents, and reared in a superior white environment. Several reports have appeared suggesting that such children grow up to be as intelligent as white children in their own families; but usually there is no control over selective placement, and the true parents could well have been of above average intelligence (see Loehlin, Lindzey and Spuhler, 1975). The most thorough study is that of Scarr and Weinberg (1976) of 99 negro or-part-negro children whose true parents were of about average education for the area. Thus the children, if reared in their own homes, would have obtained a mean IQ of about 90. Twenty-nine children were known to have 2 black parents, and when tested after the age of 4 in the foster homes, their mean was 97. Sixty-eight others with one black, one white, parent averaged 109. But white children of the adoptive parents averaged 118. Apparently then the improved environment does produce a substantial gain,

though the white-black mean difference is certainly not wiped out. Thus more data of the same kind are urgently needed. I should add that I am not taking any stand myself on the issue of racial differences in intelligence, since it is much more difficult than it is with individual differences to assess separately the genetic and environmental components. I see no reason why there should not be some innate psychological characteristics, just as there are innate physical differences, but I would expect them to be small in comparison with the very large cultural and environmental differences. Also, minority group children are much more susceptible to pre- and peri-natal handicap, and to malnutrition, than white children; and this might explain part of the difference. Unfortunately no one has yet been able to define the precise environmental factors which particularly handicap minority group children; hence environmentalist theories are almost entirely speculative. As Urbach (1974) remarked: "Everything in the world can be explained by factors that we know nothing about".

Although the total evidence of environmental effects on individual intellectual growth is very strong, the amount of improvement in IQ brought about by fostering or intervention does not exceed what would be expected from the figure of 20 to 30 per cent environmental variance that I suggested earlier. True there are occasional cases like Koluchova's twins who register far greater changes, but they are so abnormal that they can hardly be said to invalidate the heritability analyses that I referred to earlier.

Lastly, let us ask what of the future? I will be neither surprised, nor sorry, if group tests of children's intelligence disappear, particularly within elementary or primary schools. There is likely to be much less criticism by educationists and parents of instruments called Verbal or Nonverbal Reasoning tests — that is the name which Moray House adopted many years ago. Likewise the Scholastic Aptitude tests of the American College Entrance Board, or so-called factor tests such as verbal, reasoning, spatial, etc are more acceptable, and measure very much the same thing. They can be useful in rough sorting of secondary school students by ability, or for admission to American universities, and for some occupational selection as in the Armed Services. But we must discourage the notion that they measure a single, global, innate intelligence, or that they predict by themselves the educa-

tional or occupational potential of the testees. They should be regarded as only one bit of evidence whose validity for particular purposes has been demonstrated. That is, they should be combined with evidence from other sources such as scholastic or work record, interview judgments, or special aptitude tests such as mechanical, clerical, etc. Also their low reliability for long-term predictions should be borne in mind. Where the test content or format appear unfair to minority groups such as American blacks, or coloured immigrants to Britain, their validity and regression coefficients should be specially investigated. Group tests will also continue to be useful as control variables in educational or psychological researches, ie for assessing the representativeness of a population sample, or for matching contrasted samples. Since we are concerned with group characteristics in this context, the limited efficiency of the tests for individual diagnosis is less of a drawback.

Next, I don't foresee any serious problem in using individual tests like WAIS with adult clinical patients; though it would be useful to have other more diagnostically valid tests like the Reitan battery to supplement. But I am more dubious about Stanford-Binet and WISC or WPPSI for children, since these are so closely associated with the concept of innate potential. Besides the lay critics, a great many psychologists seem to think they have outlived their usefulness. The view is commonly expressed that they give an over-static picture of the developing child; they merely measure end-products, rather than psychological processes, and thus throw no light on how the retarded or maladjusted child reached that state, and how he might grow out of it. However the available alternatives which are supposed to tell us more about processes seem to consist mainly of Piagetian tasks, ITPA, etc, whose shortcomings I mentioned earlier. Moreover the well-trained Binet or Wechsler tester does find out quite a lot about processes from his qualitative observations of the child's ways of tackling items and the kinds of errors he makes. True, these judgments are subjective, and I would entirely agree that there is room for other supplementary tests to tell us more about children's cognitive styles and strategies, and specific learning disabilities, if some one would invent them. Those psychologists who simply condemn all intelligence testing could do a con-

structive and useful job by producing some better diagnostic tools.

I do not see any particular virtue in retaining the term intelligence as such, except that the general factor which runs through a wide variety of cognitive skills is too large to be ignored, and must be called something. And it does undoubtedly account for a great deal of the variance in cleverness versus dullness of children, either in everyday life, or at school. But it might be preferable to use a series of factor tests, like the McCarthy scales (1972), which yield Indices for Verbal, Perceptual-Performance, Quantitative, Memory and Psychomotor abilities. The first three of these are combined to give a General Cognitive Index. These are limited to the 2½ to 8½ year range. The new British Ability Scale (Elliott, 1974) covers from 2½ to 17 years, but it includes over 20 separately standardised tests, from which the tester can select those thought relevant to the particular child. The combined score on four designated tests yields a conventional IQ. So far there seems to be no evidence whether school or clinical psychologists regard either of these modern tests as preferable instruments to the Binet or WISC.

Several writers like Robert Glaser (1977) and Benjamin Bloom (1976) take the view that IQ tests are predictive of achievement only in a monolithic educational system. If we could provide more adaptive or individualised programmes to suit each particular child, the correlation would be much reduced, and we could make better predictions by means of a series of criterion-referenced tests, to show just what stage a child has reached in each subject, and what he is ready to go on to next. Their work is impressive, but it seems to me that its applicability is limited mainly to arithmetic, perhaps natural sciences, and early stages in English. Also I suspect that there will still be wide individual differences in overall rate of progress, and that refusal to measure this general factor would amount to throwing away the baby with the bath water. While it is true that both intelligence and educational achievement tests depend partly on genetic factors, partly on home and school environment, it is not true that they both measure the same thing. Many investigations have shown that the heritability of measured intelligence is a good deal higher than that of achievement. Intelligence refers to the general reasoning and other cognitive capacities which are developed largely by

stimulation received in the home and in leisure hours or peer-group activities; whereas achievement refers to the more specialised performance in school subjects which depends greatly on the quality of teaching and on children's motivation to learn. Nevertheless the intelligence factor gives us useful educational predictions in so far as children may usually be expected to be able to apply the reasoning capacities built up outside school to tackling any new topic in school.

This is very different from the belief that IQ tests measure innate ability, and educational tests measure acquired knowledge — a belief which is still far too commonly held by many teachers, and even by some educational psychologists. But if we accept, as I have been arguing, that genetical and environmental influences are both very much involved in the development of human intelligence, then it may be concluded that our tests, regardless of whether they are called intelligence tests or something different, will continue to be of immense value in diagnosing children's strengths and weaknesses in all areas of Special Education.

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